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Exporting Murder: US Deportations & the Spread of Violence

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Abstract

Existing literature on cross-national variation in violence has paid little attention to the transnational transmission of crime. One such channel are the forced returns of migrants with a criminal record in their countries of temporary residence. Responding to this research gap, we study the effect of US deportations of convicts on levels of violent crime in deportees' countries of origin for a cross-country panel of up to 123 countries covering the years 2003 to 2015. We find a strong and robust effect of criminal deportations on homicide rates in countries of origin, that is to a large degree driven by deportations towards Latin America and the Caribbean. An additional inflow of ten deportees with a criminal history per 100,000 increases expected homicide rates by more than two. In addition to controlling for country-specific fixed effects, we provide evidence on a causal effect using an instrumental variable approach, that exploits spatial and time variation in migrant populations' exposure to state level immigration policies in the US

Key Words: Return Migration, Deportation, Homicide, Latin America

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> "As the deportations [from the United States] have increased, so has crime, and the deportees are responsible for a disproportional amount of that…Many of the most violent offenses, like murder, kidnapping, and robbery, are committed by people who have been in the United States and are sent back here without any prior notice, with all the bad habits they developed there." Carolos Ramires Landaverde, deputy director of the department of criminal investigation in El Salvador (cited in Rohter 1997)

I. Introduction

Worldwide, deaths from intentional homicides outstrip deaths resulting from war by a large margin.[1] In 2015 the world saw five homicides per 100,000 persons; the continent of Africa experienced 13 homicides per 100,000 while Latin America had by far the largest number of homicides, with 23 homicides per 100,000 persons. While academic research on crime and internal violence has a long history, the magnitude of deaths from intentional homicide has led to a resurgence of interest in the topic amongst policymakers over the last five years. Homicides have been associated with broader patterns of social and political conflict: prolonged civil war and the endurance of failed states provide an institutional vacuum within which homicide rates have soared in African and Middle Eastern countries. This surge in violence not only continues to destabilize regional governments but also contributes to the desire of individuals to emigrate out of that continent and into the European Union. A similar story can be told about the legacy of civil conflict in the Northern Triangle of Central America: although civil wars in that region ended some two decades ago, those countries suffer from weak institutions and gang violence. Epidemic gang-related violence in Honduras, Guatemala, and El Salvador is widely understood to be a leading cause of the massive increase in the migration of unaccompanied minors to the southern border of the United States during the summer of 2014 (Clemens 2017; Renwick and Labrador 2018)

How can we explain cross-national patterns of violence? There is already a relatively large literature on the determinants of crime and criminality in comparative perspective.[2] To date,

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[1] Author's calculation using data from the World Bank's World Development Indicators. In 2015, worldwide, there were 670,285 battle deaths compared to 4,245,231 homicides. These are huge increases compared to 2010 but the magnitude of difference remains: in 2010 there were 48,952 battle deaths compared to 373,038 homicides.

[2] See Koeppel, Rhineberger-Dunn and Mack (2015) for a recent and comprehensive review of this literature.

scholars have focused on a multitude of social, economic, political, and cultural characteristics of a country when hypothesizing about the cause(s) of violent crime. We argue that these understandings, however, are incomplete because they ignore a transnational element: the role of returning criminals who have been deported from the United States back to their countries of origin. The idea that deportations from the United States are associated with an increase in violent crime in one's homeland is not new. The statement by Subcommissioner Ramirez that opens this paper has been echoed by numerous officials. Jamaica's Prime Minister P.J. Patterson argued that "the thousands of criminal deportees, mainly from the United States have added fuel to unacceptable levels of crime: Many of these persons have lived in the United States for considerable periods, been part of criminal gangs and on their return to Jamaica, link up with counterpart criminal elements to continue their illegal activities from our shores" (quoted in Griffin 2002, 42).

Viewing variation in cross-national levels of violence through the lens of deportation not only helps us understand the role of transnational factors and actors in influencing domestic behavior, it also adds to a growing literature on return migration. Deportees unlike other returnees, it must be emphasized, are not returning home voluntarily. Though, like other migrants who return home, they may bring home with them new ideas, norms, behaviors, and connections.[3] Some of these connections may be in the form of transnational criminal gangs which foster the movement of weapons, drugs, and the trafficking of human beings (Dudley 2012; Farah and Phillips Lum 2013).

We hypothesize that forced return—deportations—from the United States will, all else equal, increase violence in the deportee's homeland. Our focus on the US as source of deportations is justified for two reasons. First, the US is the largest destination for migrants in the world, with a stock of undocumented migrants of estimated at approximately 11.2 million (Passel and Cohn 2016) and cumulated amount of 5.4 million deported migrants between 1996 and

[3] The literature on migrant transnationalism and "social remittances" finds that return migration is, for the most part, associated with positive political and social change in the home country (e.g. Spilimbergo 2009; Beine, Docquier, and Schiff 2013). Our focus on deportees as a form of return migration is not intended to diminish these positive effects of migrant networks; rather it is to show that there may be a darker side to transnational ties.

2015. Of these, 2.4 million had a criminal record[4] (US Department of Homeland Security Various Years). Second, the US has relatively high levels of violence with a homicide rate more than twice the average of the group of high income countries (4.9 compared to a high-income country average of 2.2 per 100,000 according to World Development Indicators). The US is also the country with the highest per capita incarceration rates in the world (Walmsley 2013).

We face a fundamental challenge in our empirical work—that of the potential for endogeneity: Those who are deported from the United States are, by definition, migrants. And the very same push factors that lead people to leave their homes—lack of social and economic opportunities coupled with weak political institutions—are associated with crime. Deportations from the United States to country i, then, from an observational perspective, may be a future manifestation of violence in that country.

To confront this challenge, we deploy an instrumental variables strategy. In the baseline empirical model, we estimate the effect of annual per capita deportations from the US on per capita homicide rates in country i for a panel consisting of up to 123 countries covering the years 2003 to 2015. Our instrumental variable strategy exploits both spatial and time variation in migrant populations' exposure to US immigration policies as those policies vary across US states. Migrants who happen to live in US states with more restrictive immigration policies, all else equal, face a higher probability of being deported. Importantly, differences in the implementation of immigration policies across different US states are plausibly exogenous—it is difficult to envision a situation where a US state alters it's immigration policy with a view to the homicide rate in a foreign country.

The rest of the paper is organized as follows: Section II develops our argument and situates it in the broader literature. We then present the baseline model and data in section III, whereas section IV explains the instrumental variable approach. Section V presents results for the baseline model and section VI presents results for the instrumented regression. Both the instrumented and the baseline regression results show a strong and robust effect of criminal

[4] It is important to note that entering the United States illegally or overstaying one's visa does not constitute a felony; these are misdemeanors. A criminal deportation hearing is initiated for a migrant—whether they are in the United States legally or illegally—if they have committed a felony; not all migrant felons are subject to deportation hearings.

deportations on homicide rates in countries of origin: The deportation of an additional ten individuals with a criminal record per 100.000 persons in the homeland increases that country's expected homicide rates by two to three per 100.000 persons. We show, as expected, that this effect is strongly driven by the sub-region of Latin America and the Caribbean. Section VII concludes by discussing the important policy implications of these findings.

II. Deportations, Return Migration, and Violence

The literature on transnational social ties explores how migrants transmit norms, ideas, and information about their host country and community to friends and family back in their homelands. Research in this area has found positive effects of what have been called "diaspora networks" on the spread of culture, the diffusion of knowledge, and the growth of foreign investment. The literature on transnational networks, however, has difficulty distinguishing between norms and practices that are transmitted over long distances through visits, phone calls, and other temporary contacts as compared with those ideas and behaviors which a migrant brings to the homeland upon their return. In part this difficulty stems from a lack of comprehensive and comparable data on return migration—countries rarely record when the foreign born leave their shores and home countries similarly exert little effort recording who returns. That said, the scattered evidence which does exist—collected primarily from surveys—speaks to the role that returnees play in promoting democracy (e.g. Barsbai et al. 2017), promoting female political incorporation (Lodigiani and Salomone 2015), and spreading norms associated with family planning (e.g., Beine et al 2013).[5]

The literature on the effects of *involuntary* returns is even more sparse. The only literature we are aware of is comprised of qualitative stories or single cases that document how deportations are related to violence in the homeland. For the case of Central America, hypotheses on the spread of violence via deportations are based on qualitative research (e.g. Lineberger 2011; Cruz 2013; Rodgers, Muggah, and Stevenson 2009; Zuñiga Nuñez 2016) and journalistic investigations (e.g. Arana 2005; Maslin 2017) which trace the epidemic of violence to the deportation of convicted gang members from the US since the mid 1990s.

[5] See Wahba (2014) and Rapoport (2018) for reviews of this literature.

While politicians—like Ramires Landaverde and Patterson quoted above—have argued that deportations lead the violent crime, this observation does rest upon strong analytical foundations. Deportees may, all else equal, have difficulty reintegrating socially back in their homelands. Burt and Savignac (2016, 7) find the case of Honduras illustrative: "Deportees face even bleaker prospects, since, as the director of the *Centro de Atención al Migrante Retornado* (CAMR), told the authors in an interview, 'Honduran society still rejects the Hondurans that are deported, thinking that they are criminals.'" They continue: "Deportees may also be targeted by elements in the security forces. A researcher of San Diego State University has found evidence that those deported on criminal grounds are sometimes persecuted by the state."

Returnees—especially if they left their homelands due to civil conflict or economic crisis—are returning to an environment where economic, political, and social opportunities are likely limited. In some cases, gang membership provides those who return with a sense of community along with access to illicit economic opportunities (Dudley 2012). If incarcerated in the United States prior to deportation, the deportee may have been exposed to more intense criminal behavior which may alter their future behavior.[6] Those incarcerated are also more likely to have developed connections with transnational organized crime (Farah and Phillips Lum 2013). Finally, the literature on parole in the United States points to the existence of a "revolving door" whereby once released, criminal offenders who are incarcerated are more likely to commit crime once paroled than those who are sentenced to probation (e.g. Western 2006).

These conjectures have been subject to limited empirical study. To our knowledge, Blake (2014) offers the only existing quantitative study on the links between deportation and violence in countries of origin. Relying on a cross-country panel of 34 advanced and developing countries over the period 1970 to 2004, he attributes a fourth of the increase in homicide rates in developing countries in the 1980s and 1990s to the inflow of criminal deportees.

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[6] Dr. Prem Misir, Pro-Chancellor of the University of Guyana, argues that, "…criminal deportees have been intensively socialized in the criminal fields in the U.S. These deportees are in full possession of their U.S. criminal tool kit" (cited in Headley 2015).

While a useful starting point, we extend Blake's (2014) work in a number of important ways. First, we examine a much larger set of countries (123 versus 34) spanning a large universe of middle and low income countries through the year 2015. Extending through 2015 is critical as this more recent period has been characterized by heightened rates of deportation from the United States. Second, we take the issue of causality seriously and generate a plausibly exogenous instrument that exploits variation in migrants' exposure to immigration policies—at the level of the US state—which helps us decrease the possibility that our estimates are biased as a result of simultaneity or reverse causality. Finally, we embed our argument in a more comprehensive theoretical understanding of the causes of homicides[7] which allows us to hold constant a broader set of variables that are related to cross-national patterns of violent crime.[8] The next sections develop the empirical model and data used to test this hypothesis on a large panel of countries.

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[7] This literature finds its analytical roots in two canonical models. The first is Gary Becker's decision-theoretic model of the economics of crime. Becker (1968) argues that crime rates depend not just on the risks and penalties associated with the probability of apprehension but also on the risk weighted gains from criminal behavior. A second theory comes from sociological tradition and focuses on differences between in- and out-group behavior; between the haves and the have nots. The key ideas here are derived from notions of relative deprivation and focus on inequality as social and economic inequality is seen to breed social conflict (e.g. Bourguignon 2000; Ehrlich 1973; Stack 1984).

[8] The cross-national empirical literature on the determinants of violent crime—especially homicides—is mixed with regard to the importance of inequality. While Fajnzylber, Lederman, and Loayza (2002) find a statistically significant and positive effect of inequality—as measured by the gini coefficient—on cross-national homicide rates, Neumayer (2005) concludes that the statistical effect of inequality disappears once one controls for country fixed effects. Neumayer (2005), like Chu and Tusalem (2013), find that homicides are positively correlated with political instability (measured as the transition from autocracy to democracy or via measures of ethnolinguistic fractionalization) and human rights abuses.

III. The Deportation of Convicts and Homicide Rates in Countries of Origin: Data and Baseline Model

This paper's goal is to assess, empirically, whether the deportation of criminals from the US increases violence in that migrants' country of origin. Cross-country comparisons of crime pose several methodological challenges. For one, different countries may apply different definitions for what constitutes a crime or how these crimes are to be punished. Even if definitions are the same, there are likely (significant) differences in reporting biases and detection rates across countries. We expect the rate of unreported or undetected crime to differ greatly between countries and possibly also over time, depending on differences in legal systems (D'Amico and Williamson 2015) and state capacities. These observations inform our choice of dependent variable: while our argument applies to violent crime in general, we focus on homicide because homicide rates are the best observable and most comparable indicator of violent crime that suffers from the lowest bias in terms of reporting and definitions. Violent deaths are generally defined the same way across countries and over time. On the other hand, incidents of violent deaths are difficult to hide and should be subject to lower underreporting biases[9].

We utilize data from the United Nations' Organization for Drugs and Crime (UNODC) to measure homicide rates, defined as annual violent deaths per 100 thousand persons. This allows us to cover up to 123 countries for the years 2003 to 2015. As an alternative, we provide also results using homicide data from the World Health Organization (WHO) in the appendix. Although WHO data allows tracing homicide rates back to 1980 for some countries, fewer cases are covered (up to 112). Since our main focus of analysis lies on the years after 2003, we prefer UNODC over WHO data[10].

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[9] Interestingly, and contrary to expectations, the growth in prison population is negatively correlated with the growth in homicide rates. This could be due to measurement errors, or due to low clearance rates.

[10] We are also concerned about measurement errors in the WHO data, especially in earlier periods and for low and middle income countries. Since these are also the countries that received most deportees, this could produce biased coefficients.

Our explanatory variable of interest is annual deportations of criminals from the United States. Data on annual deportations by nationality of origin is available from The Homeland Security's Yearbook of Immigration Statistics (various years) which provides statistics on the number of deportations of individuals with and without criminal convictions. Figure 1 shows the total number of deportations from the US since the late 1970s. Removals intensified with passage of the Illegal Immigrant Reform and Immigrant Responsibility Act (IIRIRA) in 1996 that expanded the categories of undocumented immigrants subject to deportation and made it more difficult for them to get relief from removal. Following the IIRIA act in 1996, any "alien" who served a longer-than-a-year sentence became subject to removal from the U.S. after completion of their prison term (Cruz 2013, 5)

After a peak of more than 800 thousand annual deportations in 2013 (of these, more than 400 thousand with a prior penal history), the total number of deportees has declined in recent years, coinciding with a lower number of new arrivals and higher rates of voluntary returns to countries such as Mexico (Passel, Cohn, and Gonzalez-Barrera 2012). Over the entire period from 1978 to 2015, the total number of deported persons amounts to about 12.5 million, of these, 4.9 million had a penal record[11]. This compares to an estimated stock of undocumented migrants in the year 2014 of more than 11.2 million (Passel and Cohn 2016).

[Figure 1: Total Deportations from the US]

[Figure 2: Homicide Rates and Deportation Rates]

Figure 2 plots average levels of annual deportations of convicts per 100,000 people in origin countries against average annual homicide rates per 100,000 persons for the period 2000 to 2015, for 123 countries. The figure reveals that the relative importance of forced returns varies greatly the geographic origin of migrants, as do homicide rates. Many of the most violent countries are situated in Central America and the Caribbean, headed by Honduras

[11] It is possible that one and the same person has been deported multiple times after re-entry in the US. We are therefore not able to give a prices estimate on how large the cumulative number of deportees (excluding double-counts) is.

(67), El Salvador (61) and Jamaica (49). Peak years for homicide rates are even higher, reaching 108 in El Salvador and 93 in the gang-ridden countries El Salvador and Honduras.

Regarding annual deportations of convicts, the countries which received the largest number of convicted returnees relative to home country population are Mexico, the Northern Triangle of Central America (El Salvador, Honduras, Guatemala) as well as Belize and Jamaica. By 2015, the cumulated number of deported convicts in the most affected countries had reached 95 thousand in El Salvador, 115 thousand in Honduras and > 1.7 million in Mexico. The cumulative number of deportees with a criminal background correspond to between 1.3% and 1.5% of these countries' population stocks in 2015. It is clearly conceivable that such as sizable number of convicted deportees could have affected violence at countries of origin, suggested not only by the correlation in Figure 2 but also by anecdotal and journalistic evidence from Central America . In the following, we explore whether these two factors—deportations and homicides—are linked in a causal way.

As a first step, we estimate the following baseline model:

$$(1)\ HomicideRate_{i,t} = \beta_1\ DeportationRate_{i,t} + \beta_2\ X_{i,t} + v_i,$$

where $HomicideRate$ are annual homicide rates for country i in year t are annual convicted deportees per home country population received by country i. The time period t refers to two-year intervals. The main reason is that deportations may not necessarily have an immediate effect on rates of violence upon arrival. Instead, they may affect the outcome variable with a time lag. By using a panel of two-year intervals we allow for longer lags in the effect compared to annual data. Two-year intervals proved to be empirically strongest and allowed us to maintain a sufficient number of observation over time while at the same time providing some flexibility in the timing of the effect. Homicide rates and deportations are calculated over two-year means (i.e. as averages over the current and the preceding year). Results for 1 and 3-year intervals are shown in the appendix.

All regressions reported below control include a set of country fixed effects to account for unmeasured, or unobservable, factors that may influence both deportations and homicide rates. A country's history of civil conflict, for example, may have left that country with a stock of arms and a legacy of violence that may pose more fertile grounds for a posterior increase of homicide rates. Country fixed effects may also capture the propensity of a

domestic population to self-select into more marginalized neighborhoods due, for example, to cheaper housing costs. The existence of these neighborhoods may expose migrants and their children to higher levels of violence and crime which, in turn, would make them more vulnerable to criminal careers and posterior deportations. Country specific fixed effects may also capture slow moving institutional and cultural factors in a country that are difficult to observe but may, nonetheless, influence rates of interpersonal violence.

In Equation 1, X is a vector of time-varying control variables, that have been identified as potential predictors of cross-country variation in violence in the empirical literature. Following this literature, we include a number of variables: average socioeconomic status which we measure using both the log of per capita income and the country's aggregate level of GDP growth. Income is an imperfect measure of development so we also include the average level of education measured as the average years of schooling of the adult population. These variables assume that changes in homicide rates are potentially related to levels of economic and human development. We include the Gini index as a measure of income inequality, because we expect countries with large income differentials to suffer from higher levels of social conflict and violence, For a similar reason, a dummy variable measures whether countries where in a civil war during the period in question. As demographic indicators, we also include the log of population size, the share of population under the age of 14, and the share of the population living in urban areas (e.g. Fajnzylber, Lederman, and Loayza 2002; Neumayer 2005; Chu and Tusalem 2013 as cited above).

Regarding policy variables, we control for how democratic a country is, ranging from most authoritarian (-10) to most democratic (10) and include an integrated measure of corruption taken from the World Bank's Governance Indicators; this measure captures the perception of government corruption in a country. Small arms imports from the US (measured as per capita units) to each country i controls for alternative potential channels that could affect homicide rates. We also add remittances received relative to home country GDP as an indicator for country's dependence on out migration.

Finally, we include an indicator of the exposure of the diaspora's—their migrant population living in the United States-- average exposure to crime in the United States in order to control for the potential that the diaspora is transmitting norms or practices regarding violence back to their homeland. To this end, we measure annual violent crimes per capita at the US state level and then weigh state-level crime rates by the distribution of the foreign born population

from each origin country i in each US state j. See Appendix Table 1 for descriptive statistics and sources of all variables.

Although deportations from the US can be traced back to 1978, our empirical analysis focuses principally on the period 2003 to 2015, for several reasons. For one, deportation rates from the US actually became quantitatively important only in the mid 1990s, as illustrated in Figure 1. Second, our analysis is limited by the availability and quality of the dependent as well as of control variables. In order to limit the chance that our inferences are influenced by poor measurement and noncomparable country samples, we focus on the most recent decades. Finally, the instrumental variable strategy developed below relies on information available for the period after 2000 only. We therefore focus our analysis on the period from 2003 to 2015. Results based on WHO data are provided in the appendix.

IV. Identification Strategy: Using State-Level Variation in Immigration Policies as a Source of Exogenous Variation

We are interested in estimating a causal effect of deportations from the United States on homicide rates in the deportee's country of origin. In spite of including country fixed effects in equation (1), a causal interpretation of the coefficient on deportation rate would be problematic under certain conditions. On the one hand, we may observe reverse causality, for example when an increase in violence in countries of origin also triggers higher rates of emigration that might in turn may lead to higher deportation rates. Others might argue that a rise in violence at origin could be reflected in different migrant characteristics or a different behavior of migrants, that would make them more prone to engaging in criminal activities. In both cases, sceptics might claim that an increase in origin countries' rates of violence could translate into a higher number of deportations, with causality running from a growth in violence at origin to a growth in deportation rates. A second concern with equation (1) relates to the potential for omitted variable bias. Both deportations and origin countries' rates of violence might respond to a third omitted variable we are not able to observe. For example, networks of transnational crime—which are directly unobservable from a quantitative perspective—may affect levels of violence in both the countries of origin and the country of residence. In this case deportations and violence at in the country of origin could move in parallel without being related in a causal way.

To address these concerns, we use an instrumental variables approach to estimate equation (1) via two-stage least squares. Our instrument exploits spatial variation in immigration policies across US states as a source of exogenous variation that explains variation in deportation rates by country of origin, but is not (directly) related to changes in violence at countries of origin. The rationale for the instrumental strategy is as follows: While the US Constitution charges the federal government with the creation of laws governing who can enter the United States and requirements for when foreigners can become citizens, individual states have the legal right to determine when and how non-citizens can participate in the labor market, when they, when they can attend local schools, and the conditions under which they can obtain local services. We argue that when migrants—regardless of status—live in states that employ a tougher stance with respect to undocumented migrants they are more likely to be deported upon the commission of a felony. The toughness of the state's stance regarding migrants should be correlated with the US's aggregate deportation rate but, at the same time, should not directly influence the homicide rate in the migrant's country of origin other than through the deportation rate.

We measure the toughness of a state's immigration policy with three binary variables. First, we use an indicator for whether states participated in *Secure Communities*, a federal data-sharing program through which fingerprints submitted by local law enforcement agencies to the FBI are shared with immigration enforcement agencies for checks against immigration databases. Depending on the result, immigration officials decide whether to take enforcement action, like issuing a detainer request. A second indicator reports whether states had a policy to mandate that some or all employers use *E-Verify*, an electronic verification system that confirms the employment eligibility of workers and therefore excludes undocumented migrants from formal labor markets. Third, we use an indicator for whether each state provides prenatal care, regardless of a woman's migratory status, through either a state-funded program or through a 2002 Children's Health Insurance Program (CHIP) option. All three indicators are taken from Gelatt, Bernstein and Koball (2018) and cover the years 2000 to 2016[12].

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[12] Among a larger set of policies targeted towards undocumented immigrants, these three proved to be the strongest predictors for deportation rates. Other possible variables include access to education for undocumented migrants, different health and social benefits, and different forms of cooperation in terms of immigration enforcement policies. See Gelatt,

Each of the instrumental variable varies by groups of immigrants i according to their exposure to immigration policies at the state level. We obtain a different indicator for each immigrant group i by weighting the state-level indicator on immigration policies with immigrant group's share of migrants living in US state j. Note that all annual variation in the instrumental variable is attributed to period-to-period changes in immigration policies at the level of US states. The spatial distribution of migrants refers to cross-sectional distribution of the foreign-born population extracted from the American Community Survey for the years 2005-2009 (US Census Bureau 2011). Due to the importance of networks to reduce costs of migration, migration routes tend to develop along specific corridors that are very stable over time (McKenzie and Rapoport 2007). Variation in migration corridors and, as a result, different exposures to conditions in migrants' countries of residence has frequently been used by migration researchers as a source of exogenous variation in instrumental strategies (e.g. Yang 2008; Adams and Cuecuecha 2010; Ambrosius and Cuecuecha 2016; Anzoategui, Demirgüç-Kunt, and Martínez Pería 2014).

The first stage instrumental regression can be formulated as follows:

$$(2)\ DeportationRate_{i,t} = \beta_1 ImmigPolicy\_State_{i,t-1} + \beta_2\, X_{i,t} + v_i,$$

As in eq. (1) above, $DeportationRate$ are deportations for each country i relative to the population size of the country of origin. $ImmigPolicy\_State$ refers to three instrumental variables as described above. To ensure that the instruments precede deportations and are not themselves affected by deportation rates, we use the lagged values of the instrument in $\mathrm{t}-1$. X refers to the same vector of control variables as explained above, and v is the set of country fixed effects. In the second stage regression, we replace $DeportationRate$ with the estimated values $\widehat{DeportationRate}$ as obtained from eq. 2; this provides us with estimates of the country- and time-specific deportation rate which are uncorrelated with homicide rates in the

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Bernstein and Koball (2018) for details and an overview of other state-level immigration policies (URL: https://urbn.is/2vLfY5Z).

deportee's homeland. This enables us to obtain a causal estimate of the effect of deportation on homicide rates.

The validity of our instrument rests on two assumptions. First, the instrument has to be relevant, i.e. state-level variation in deportation policies has to be a sufficiently strong predictor for the deportation of convicts. We expect countries whose expatriates live in US states with more hostile immigration policies to suffer from much larger threats of deportation. This assumption will be tested formally below.

Second, the instrument has to be excludable, i.e. state-level variation in deportation policies should be exogenous and not (directly) related to violence in countries of origin. Although this assumption cannot be formally tested, it should hold as long as state-level variation in deportation policies does not respond to migrants' behavior or changes in rates of violence in countries of origin, for example because states respond to a surge in criminal engagement of specific migrant populations that are concentrated in their jurisdictions. In order to ensure that the effect is not driven by migrants' characteristics, regressions include controls for social, economic and demographic characteristics of countries of origin. The vector of control variables in X also includes an indicator for the average exposure to state-level violent crime of the migrant population. This assures that results are not driven by higher levels of crime at migrants' places of residence. Moreover, we use lagged values of the instruments. Lagging instruments by one period ensures that homicide rates and subnational variation in deportation policies do not respond to unobserved simultaneous events in t=1 or the possibility that deportation policies respond to current rates of violence in countries of origin. Together with country fixed effects, state-level variation in US immigration policies should provide a strong and exogenous instrument: there is no reason to believe that regional variation in immigration policies bears any direct effect on the origin countries' homicide rates in the following period, other than through the deportation of convicts.

V. Effects of Deportations on Violence: Results from Panel Data Analysis

Before turning to the instrumental variables analysis, in Table 1 we present five different empirical specifications corresponding to equation (1). All of the specifications use country as well as time fixed effects, hence controlling for time-invariant country characteristics and

eventual time trends in the variables. As mentioned above, the panel uses two year intervals on mean values, both for homicide rates and deportation rates.

The first column in Table 1 shows result for convicts' deportations without additional controls. The second column adds additional time-varying controls as discussed above. The third column adds the rate of non-criminal deportations. The fifth to seventh column repeat column two's specification on three subsets: Column 5 assess whether these results hold when excluding deportees to high income OECD countries from the sample. Although this group of countries can be seen as a control group that increases the number of observations, deportation rates are actually negligible for high-income countries. Hence, we do not expect deportations to have any measurable effect on origin country violence for this group. The last column repeats regression for the subset of Latin American and Caribbean countries. We expect results to be largely driven by migrants from the Latin American subcontinent: The Diaspora especially from Central America and the Caribbean is to a large degree concentrated in the US, and the Latin American and Caribbean region also suffered from the highest deportation rates. Moreover, Latin America constitutes a more homogenous group of countries compared to the rest of the world. This allows us to assess the effects of deportation rates on a group of countries that is relatively homogeneous both in terms of colonial and historical legacies of covariates we might not be able to fully observe. Column seven excludes Latin America from the sample.

Few of the control variables in Table 2 show a strong and statistically significant pattern. Larger population sizes are associated with higher homicide rates. Higher rates of schooling are associated with lower homicide rates. Countries with a younger population structure tend to report higher homicide rates, although this demographic indicator is statistically significant only for the subset of Latin America. It is noteworthy that migrants' exposure to crime at destination is, if anything, negatively related to homicide rates at origin. This supports our view that it is deportations rather than a shared trend in violence at places of residence and origin that explains our findings. Interestingly, arms exports from the US are related to lower, not higher homicide rates, but is statistically significant only for the Latin American subset.

The deportation of convicts has a strong and consistent positive effect on homicide rates in countries of origin in all specifications, with a coefficient that ranges between .18 and .27 in specifications 2 to 6, and a somewhat lower coefficient in the first column without any control variables. We do not observe the same effect for deportation rates of non-convicts. In column 3, this variable is statistically indistinguishable from zero while the criminal deportation rate

continues to be statistically significant and positive. This suggests that it is not deportations per se that are driving homicides; rather criminal history of the individual deported is driving homicides. Column 4 provides additional support for this conjecture as the non-criminal deportation rate continues to be statistically insignificant even after dropping the criminal deportation rate.

Results from Table 1 imply that an inflow of ten additional deportees with a criminal record per 100 thousand people increases expected homicide rates by roughly two. Put differently, a one standard deviation increase in the 2012 deportation rates (≈ 24) is associated with an increase in homicide rates of ≈ 4.3 per 100 thousand, based on the point estimate in column 2 (24 * .18 ≈ 4.3). For Honduras, the country with the highest deportation rate (≈162) as well as the highest homicide rates (≈ 93 per 100 thousand) in 2012, the point estimate assigns roughly a third of its predicted homicides to forced deportations of convicts (162 * .18 ≈ 29, corresponding to ≈ 30% of its 2012 homicide rate).

Coefficients are similar in size and statistical significance for the subset excluding high income OECD countries as well as for the much smaller subset of Latin American and Caribbean countries. However, when excluding Latin America, the effect vanishes (column seven). This supports our expectation that the observed effect is to a large degree driven by experiences of the Latin American subcontinent that provides the largest variation in terms of homicide rates as well as deportation rates.

[Table 1: Effect of US Deportations of Convicts on Origin Countries' Homicide Rates]

VI. Instrumental Variables Model

A causal interpretation of coefficients in Table 1 could be questioned on several grounds: violence may give rise to a surge in out-migration (e.g., Clemens 2017) which may, in turn, give rise to an increased border security in the United States, a strategy which may include increased deportation rates. Or, it may be the case that both US immigration policy and violence in a country of origin may be responding to a third, unobserved factor such as the growth of transnational organized crime.

We confront these potential challenges to inference via an instrumental variables strategy using two-stage least squares. We argue that that migrants' exposure to state-level variation in immigration policies provide a source of exogenous variation that is correlated with criminal deportation rates. As discussed above, we use three instrumental variables to capture the exposure of migrants to immigration policies along three dimensions: Whether states participated in the data-sharing program "Secure Communities", whether private or public employers had to use "E-verify" to check employment eligibility, and whether child and health care benefits extended to undocumented pregnant women. Each country's diaspora in the United States—measured in terms of the size of the foreign born population from country i residing in the United States at time t—is weighted by its exposure to these state-level immigration policies.

Table 2 provides results for the first stage model. The instrument of lagged immigration policies has strong joint and individual effects on criminal deportation rates. F-statistics (Chi square) for their joint significance are above 20 in all specifications (see test statistics in Table 3) which is above the benchmark critical value for weak instruments (Stock and Yogo 2002). The instrumental variable "secure communities" reflects the enforcement of a policy that is directly targeted towards undocumented migrants who have broken the law. The instrument is positive and statistically significant: Migrant communities exposed to the enforcement of the policy face higher deportation rates of convicts. Interestingly, the other two instruments that are targeted towards labor markets (E-verify) and benefits for undocumented pregnant immigrants (prenatal care) seem to operate in a different way: Exposure to more restricted labor market has a negative effect on convicts' deportation rates, whereas more generous benefits for pregnancy and pre-natal care are associated with higher rates of convicts' deportations. One explanation for the latter results is that the enforcement of deportations, the exclusion from labor markets and the extension of social benefits could pose trade-offs for policymakers: A more generous stance in labor markets and social policies could be justified by a strict application of "zero tolerance" deportation policies to those who have committed crimes.

As above, all specifications maintain year and time fixed effects, hence all coefficients are estimated from within-country variation (i.e. changes in deportation rates over time). Control variables from the first-step regression indicate that migrants from countries that are poorer, smaller, more unequal and have a younger age structure experience more deportations. As expected, migrants that are exposed to higher levels of violent crime at their place of residence also experience higher deportation rates. In line with expectations, migrants from

countries that are characterized by more precarious conditions are also deported at higher rates.

Results from the 2nd stage regression in Table 3 repeat regressions from the previous section but replace the variable $DeportationRate$ with predicted values $\widehat{DeportationRate}$ from first step regressions in Table 2. The instrumental variables models provide surprisingly consistent and robust results: All instrumented point estimates for the rate of deported convicts lie in the range between .23 and .3, a slightly larger effect compared to the un-instrumented regressions results. In fact, a Wu-Hausman-test on the coefficient for deportations does not reject the null-hypothesis of endogeneity (i.e. there is no sign that coefficients in the instrumented regressions are significantly different from the uninstrumented coefficients). The signs of control variables are similar to those in Table 2 with only minor differences in terms of size and statistical significance. As in Table 2, results hold for the subsets excluding high income OECD countries and for the sample of just Latin American countries[13]. In sum, the use of a lagged instrumental variable together with country fixed effects and a large number of control variables confirms the existence of a strong causal relationship between the deportation of convicts and homicide rates in countries that receive deportees. The Sargan over-identification test is not rejected, confirming the validity of the instruments (i.e. all three instruments are exogenous and uncorrelated with the model residuals). Instrumented results are robust to the use of different indicators, samples and intervals. The table in Annex 2 uses data from WHO instead of UNODC data on homicides for a smaller cross-sectional sample. Findings are robust to the use of alternative data. Tables in Annex 3 and 4 use one and three-year intervals instead of two-year intervals. Whereas some of the coefficients lose statistical significance when using three-year intervals, the main message remains unaltered.

[Table 2: Effect of Exposure to State-Level Immigration Policies on Convicts' Deportations from the US (1st Step Instrumental Regression)]

[13] We do not show results for the subset excluding Latin America and the Caribbean (as column seven in Table 1), because the instrument is too weak when excluding the Latin American sub-region.

[Table 3: Instrumented (2nd Step) Regression: Effect of US Deportations of Convicts on Origin Countries' Homicide Rates]

VII. Conclusions

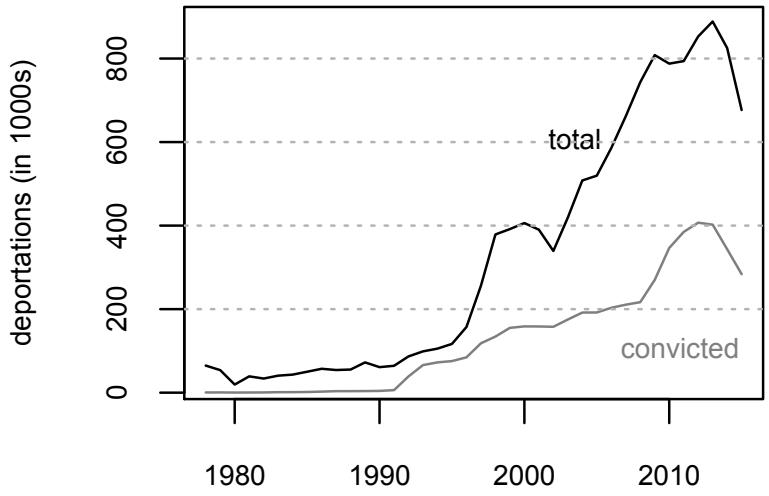
We examine the effect of return migration on crime in the returnee's home country. Focusing on the effect of returning criminals who have been deported from the United States, we observe a strong and robust effect of criminal deportations on violence in migrants' countries of origin. The causal effect is striking: for every ten deportees per 100,000 persons, we observe an increase of homicide rates by more than two. These results are to a large degree driven by Latin America and the Caribbean, the region with the largest variation and the largest levels both in deportation inflows and homicide rates. Using an instrumental variable approach where we use the exposure of migrant populations to variation in immigration policies as an exogenous source of variation backs the causal interpretation of our results.

These empirical findings have several important implications. First, our study emphasizes the transnational roots of violence; roots that have largely been neglected in the quantitative empirical literature. Second, it highlights one channel through which emigration may produce negative long-term social outcomes at origin. Most importantly, the findings carry strong policy implications. The deportation of convicts bears huge follow-up costs for countries receiving deportees. These countries generally provide few perspectives for returnees in terms of social and economic reintegration and lack adequate institutions and resources to respond to imported violence. Last but not least, deportation policies tend to provoke new emigration. Violence in Central America - that our empirical model at least partly attributed to deportations from the US - have led to a new wave of immigration to the US, particularly of unaccompanied minors (Clemens 2017; Renwick and Labrador 2018)

Recently, forced returns have been applied to migrants from Islamic countries in Europe and elsewhere that have been suspect of constituting terrorist threats. One limitation of our study is that we are able to estimate effects of criminal deportations from the US only, whereas data restrictions do not allow us to assess the effect of deportations along other migration corridors. Even so, our results should also be read as a warning against the possible consequences of forced returns in other contexts and situations.

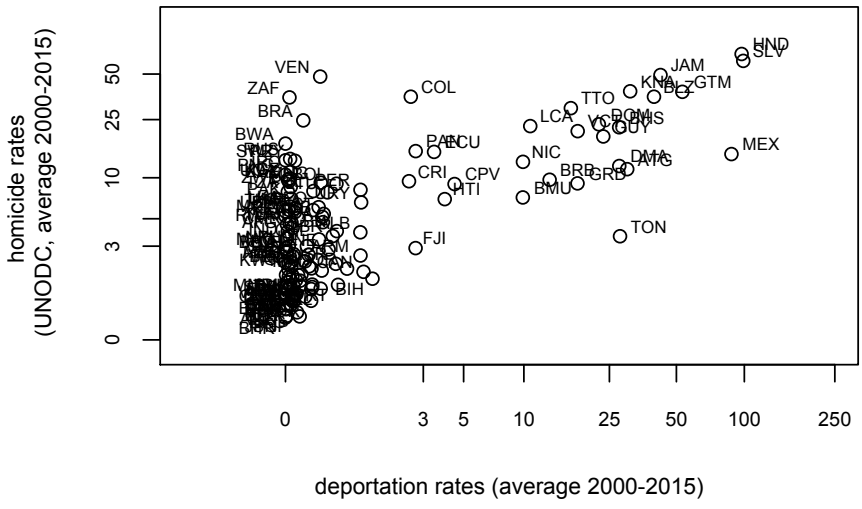
VIII. Tables

Figure 1: Annual Deportations from the US (Total and Deportees with Penal Record)



Annual deportations based on data from US Department of Homeland Statistics, Yearbooks of Immigration Statistics (various years)

Figure 2: Homicide Rates and Deportation Rates



Homicide rates and deportations rates per 100,000, based on data from United Nations Organization for Drugs and Crime and US Department of Homeland Security

Table 1: Effect of US Deportations of Convicts on Origin Countries' Homicide Rates

	homicide rate						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
deportation rate (convicts)	0.15**	0.18**	0.23**		0.19**	0.27***	-1.1
	[2.34]	[2.33]	[2.12]		[2.55]	[3.46]	[-0.89]
deportation rate (non conv.)			-0.058	0.018			
			[-1.22]	[1.06]			
civil war		-1.5	-1.1	-0.18	-0.78		0.22
		[-1.57]	[-1.25]	[-0.19]	[-0.63]		[0.45]
corruption		-0.37	-0.26	-0.3	-0.46	-0.56	-0.027
		[-0.72]	[-0.51]	[-0.53]	[-0.41]	[-0.3]	[-0.12]
migrants' exposure US crime		-0.041	-0.032	-0.063	-0.06	-0.14**	0.041
		[-1.41]	[-1.23]	[-1.36]	[-1.55]	[-2.44]	[1.22]
GDP growth		1.3	0.49	-5.7	4.6	11	-0.0072
		[0.58]	[0.23]	[-1.38]	[0.97]	[0.69]	[-0.36]
GDP per capita		-0.035	-0.022	0.0079	-0.11	-0.32*	-0.93
		[-0.61]	[-0.4]	[0.16]	[-1.15]	[-1.67]	[-0.5]
gini (market)		0.16	0.054	0.0028	0.31	0.77	0.17*
		[0.94]	[0.35]	[0.02]	[0.88]	[1.53]	[1.94]
polity		0.023	-0.0092	0.048	0.1	0.4	0.17
		[0.24]	[-0.12]	[0.56]	[0.89]	[1.23]	[1.58]
population (log)		10	11**	6.5	15	26	3.9
		[1.51]	[1.95]	[1.34]	[1.64]	[0.47]	[0.92]
remittances		-0.34	-0.026	-0.38	-0.41	-1	-0.18
		[-0.61]	[-0.1]	[-0.77]	[-0.69]	[-0.95]	[-1.55]
schooling		-0.56	-0.72*	-0.88	-1.4**	-1.4**	-0.051
		[-1.36]	[-1.8]	[-1.65]	[-2.46]	[-2.54]	[-0.69]
share population < 14		0.81	0.69	-0.35	0.8	5.3***	-0.28
		[1.52]	[1.45]	[-0.54]	[1.12]	[2.84]	[-1.28]
urban		0.21	0.31	0.41	0.24	0.56	-0.13
		[0.93]	[1.41]	[1.47]	[0.6]	[1.05]	[-0.76]
US arms imports		-1.50E-04	1.60E-04	-0.0001	-0.022	-0.026*	-0.0004
		[-0.34]	[0.41]	[-0.17]	[-1.46]	[-1.79]	[-1.42]
sample	full	full	full	full	w/o OECD	Lat. Am. & Carib.	w/o Lat. Am. & Carib.
country fixed effects	yes	yes	yes	yes	yes	yes	yes
year fixed effects	yes	yes	yes	yes	yes	yes	yes
R^2	0.16	0.3	0.35	0.14	0.36	0.48	0.28
F-stat	15.29	5.87	6.37	1.98	3.87	2.83	3.88
#obs	608	358	337	337	198	93	265
periods (max)	6	6	6	6	6	6	6
countries	123	78	78	78	50	21	57

Heteroscedasticity robust t-values in parenthesis. All results are based on ordinary least squares regressions with country and year fixed effects for 2-year intervals. Stars denote significance at the 10% (*), 5% (**) and 1% (***) level.

Table 2: Effect of Exposure to State-Level Immigration Policies on Convicts' Deportations from the US (1st Step Instrumental Regression)

	deportation rate (convicts)				
	Spec.1	Spec.2	Spec.3	Spec.4	Spec.5
secure communities (lagged)	9*	19*	16*	21	23
	[1.94]	[1.7]	[1.71]	[1.22]	[1.21]
e-verify (lagged)	-9.4	-57***	-31**	-52**	-48**
	[-1.03]	[-2.8]	[-2.25]	[-2.5]	[-2.49]
prenatal care (lagged)	52*	150***	110***	200***	270***
	[1.95]	[3.28]	[2.81]	[3.95]	[4.97]
deportations (non convicts)			0.35***		
			[12.08]		
civil war		6.7	2.5	8.2	
		[1.47]	[0.89]	[1.46]	
corruption		-1.8	-1.5	-1.2	-1.9
		[-1.2]	[-1.59]	[-0.51]	[-0.71]
migrants' exposure US crime		0.0028	-0.0029	0.098	0.39***
		[0.04]	[-0.06]	[0.99]	[3.52]
GDP growth		-0.0082	0.028	0.13	0.38
		[-0.09]	[0.5]	[1.23]	[1.65]
GDP per capita		-29***	-17***	-43***	-65***
		[-2.78]	[-2.7]	[-3.34]	[-3.03]
gini (market)		-1.2	-0.64	-1.4	-2.9*
		[-1.54]	[-1.57]	[-1.33]	[-1.73]
polity		-0.032	0.018	-0.21	0.85
		[-0.07]	[0.07]	[-0.41]	[0.6]
population (log)		-19	-23*	-51*	-53
		[-0.83]	[-1.75]	[-1.91]	[-0.36]
remittances		1.2**	-0.45	1**	2.1***
		[2.19]	[-1.25]	[2.13]	[3.65]
schooling		0.56	0.062	0.6	2.5
		[0.87]	[0.15]	[0.41]	[1.63]
share population < 14		-3.8***	-2.1***	-4***	-7.8***
		[-3.04]	[-2.76]	[-2.76]	[-2.57]
urban		0.23	0.17	0.21	1
		[0.48]	[0.59]	[0.3]	[0.71]
US arms imports		0.00075	-0.00034	-0.062*	-0.039
		[0.32]	[-0.32]	[-1.77]	[-1.05]
sample	full	full	full	w/o OECD	Lat. Am & Caribbean
country fixed effects	yes	yes	yes	yes	yes
year fixed effects	yes	yes	yes	yes	yes
R^2	0.08	0.47	0.72	0.56	0.77
F-stat	7.47	14.17	36.82	11.01	13.89
#obs	868	458	430	264	129
periods (max)	7	7	7	7	7
countries	126	81	81	53	22

Heteroscedasticity robust t-values in parenthesis. All results are based on ordinary least squares regressions with country and year fixed effects for 2-year intervals. Stars denote significance at the 10% (*), 5% (**) and 1% (***) level.

Table 3: Instrumented (2nd Step) Regression: Effect of US Deportations of Convicts on Origin Countries' Homicide Rates

	homicide rate				
	(1)	(2)	(3)	(4)	(5)
deportation rate (convicts)	0.23***	0.24***	0.27***	0.21***	0.3***
	[3.14]	[3.04]	[2.73]	[2.91]	[4.77]
deportation rate (non conv.)			-0.07		
			[-1.55]		
civil war (=1)		-1.9*	-1.2	-0.96	
		[-1.66]	[-1.26]	[-0.74]	
corruption		-0.38	-0.26	-0.47	-0.56
		[-0.7]	[-0.5]	[-0.41]	[-0.3]
exposure US crime		-0.034	-0.027	-0.059	-0.14**
		[-1.18]	[-1.03]	[-1.55]	[-2.45]
GDP growth		3.5	1.4	5.7	14
		[1.06]	[0.54]	[1.14]	[1.01]
GDP per capita		-0.044	-0.026	-0.12	-0.32*
		[-0.74]	[-0.49]	[-1.21]	[-1.74]
gini (market)		0.21	0.062	0.33	0.85
		[1.07]	[0.39]	[0.91]	[1.64]
polity		0.013	-0.018	0.1	0.31
		[0.11]	[-0.2]	[0.85]	[0.88]
population (log)		11	11**	16	24
		[1.38]	[2.01]	[1.64]	[0.42]
remittances		-0.33	0.027	-0.41	-1.1
		[-0.57]	[0.1]	[-0.68]	[-1.01]
schooling		-0.55	-0.69*	-1.3**	-1.4**
		[-1.39]	[-1.81]	[-2.42]	[-2.48]
share population < 14		1.2*	0.85	0.95	5.7***
		[1.87]	[1.63]	[1.2]	[2.74]
urban		0.17	0.3	0.22	0.48
		[0.75]	[1.32]	[0.53]	[0.85]
US arms imports		-0.00018	0.0002	-0.02	-0.023
		[-0.41]	[0.53]	[-1.36]	[-1.63]
sample	full	full	full	w/o OECD	Lat. Am & Caribbean
country fixed effects	yes	yes	yes	yes	yes
year fixed effects	yes	yes	yes	yes	yes
R^2	0.16	0.29	0.35	0.36	0.48
weak instr. F-test	26.69	26.27	26.05	21.39	21.39
Wu Hausman	0.03	0.14	0.44	0.82	0.82
Sargan	0.88	0.88	0.88	0.88	0.88
#obs	568	358	337	198	93
periods (max)	6	6	6	6	6
countries	113	78	78	50	21

Heteroscedasticity robust t-values in parenthesis. All results are based on 2SLS regressions with country and year fixed effects for 2-year intervals. Stars denote significance at the 10% (*), 5% (**) and 1% (***) level.

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X. Appendix

Annex 1: Data Description and Sources

Variable	Description	mean [st.dev.]	# obs
deportation rates (convicts)	Annual inflow of deported persons from the US with a penal record, per 100,000 of home country population. Two-year rolling means [a)]	5.70 [17.37]	897
deportation rates (non convicts)	Annual inflow of deported persons from the US without a penal record, per 100,000 of home country population. Two-year rolling means [a)]	7.20 [30.32]	764
homicide rates (WHO)	Homicides per 100,000 persons, according to data of the World Health Organization, covering the years 1980 to 2015. Two-year rolling means [b)]	7.21 [10.18]	464
homicide rates (UNODC)	Homicides per 100,000 persons, according to data of the United Nations Organization for Drugs and Crime (UNODC), covering the years 2013 to 2015. Two-year rolling means [c)]	8.73 [13.07]	614
civil war	Binary indicator whether the country was in a civil war in the current year [d)]	0.01 [0.09]	983
corruption	A composite measure of corruption within the political system considered a threat to foreign investment by distorting the economic and financial environment, reducing the efficiency of government and business by enabling people to assume positions of power through patronage rather than ability, and introducing inherent instability into the political process. Ranges between 0 (most corrupt) and 6 (least corrupt) [e)]	2.59 [1.17]	821
exposure US crime	Average exposure of migrant population from country i to violent crime known to police, including murder & non-negligent manslaughter, legacy rape, revised rape, robbery, and aggravated assault at the level of US states. Average exposure is calculated from the distribution of the foreign born population across US states according to the 5-year American Community Survey (ACS) 2005 to 2009 [h) i)]	441.99 [51.72]	762
GDP growth	Per capita annual GDP growth [f)]	4.39 [4.74]	891
GDP per capita	Per capita GDP in constant 2010 USD (logs) [f)]	8.47 [1.45]	889
gini (market)	Gini indicator based on market income [g)]	45.67 [6.21]	729
polity	Regime type indicator, ranging from most authoritarian (-10) to most democratic (+10) [j)]	4.38 [5.99]	765
remittances	Migrants' remittances as share of GDP [f)]	4.92 [6.91]	823
schooling	Average years of schooling of the adult population [k)]	8.30 [2.83]	709
share population < 14	Share of the population under the age of 14 [f)]	28.94 [10.90]	875
urban	Share of the urban population. Yearly values interpolated from quinquennial data. [l)]	57.73 [23.80]	1206

US arms imports	Import of non-military arms from the US, as units per 100,000 of home country population [m)]	44.59 [133.39]	571
population (log)	Population size (logs) [f)]	15.93 [2.02]	908
secure communities (lagged)	Instrumental variable, weighting the foreign born population from country i that lived in US states that operated the program "Secure Communities". Secure Communities is a federal data-sharing program that ran from 2008 to 2014 and was reinstated in January 2017. State participation was initially voluntary but later became mandatory and was active in all states by 2012. Through Secure Communities, fingerprints submitted by local law enforcement agencies to the FBI are shared with immigration enforcement agencies for checks against immigration databases. Depending on the result, immigration officials decide whether to take enforcement action, like issuing a detainer request. Indicator is lagged by one year [n) i)]	0.31 [0.40]	766
e-verify (lagged)	Instrumental variable, weighting the foreign born population from country i that lived in US states that operated the program "e-verify". E-verify is an electronic verification system that confirms the employment eligibility of workers. Most states with this mandate require either public employers and state contractors, or all employers with at least a certain number of employees, to use E-Verify. Takes the value 1 in states where employers are mandated to use E-Verify for some hires and takes the value 2 in states where E-Verify was mandated for all hires. Indicator is lagged by one year [n) i)]	0.09 [0.12]	766
prenatal care (lagged)	Instrumental variable, weighting the foreign born population from country i that lived in US states that provided prenatal care, regardless of a woman's residency status, through either a state-funded program or through a 2002 CHIP (Children's Health Insurance Program) option. Indicator lagged by one year [n) i)]	0.69 [0.14]	766

Mean values, standard deviations in brackets and the number of observations are provided for the two-year intervals between 2003 and 2015. Sources: [a)] US Department of Homeland Security. Yearbook of Immigration Statistics. Various Years (URL: https://www.dhs.gov/immigration-statistics/yearbook) [b)] World Health Organization (WHO). Historical trend series data on deaths from road traffic accidents, suicide and homicide (URL: http://www.who.int/violence_injury_prevention/surveillance/databases/mortality/en/) [c)] United Nations Organization on Drugs and Crime (UNODC). Data series on homicide and other criminal offences (URL: http://www.unodc.org/unodc/en/data-and-analysis/statistics.html) [d)] Sarkees and Wayman (2010)[e)] International Country Risk Guide. The PRS Group (URL: https://www.prsgroup.com/) [f)] World Development Indicators Online Database [g)] Solt (2016) [h)] US Department of Justice-Federal Bureau of Investigation, via US Census Bureau (2017) [i)] American Community 5-Year Survey 2005 – 2009 [j)] Integrated Network for Societal Conflict Research (INSCR), Polity IV Project, Political Regime Characteristics and Transitions, 1800-2016 [k)] Barro and Lee (2001) [l)] United Nations Population Division. World Urbanization Prospects 2017 (URL: https://esa.un.org/unpd/wpp/) [m)] United Nations COMTRADE Database (URL: https://comtrade.un.org/data/) [n)] Gelatt et al. (2018). Original sources for the three indicators on state-level migration policies are Medical Assistance Programs for Immigrants in Various States, National Immigration Law Center, Issues: Health Care, 2002, 2005–07, 2009–12, 2014–16, for health benefits; US Immigration and Customs Enforcement, Freedom of Information Act (FOIA) Library: Secure Communities Nationwide Interoperability Statistics (FY 2009, 2010, 2011, 2012, 2013, 2014, and YTD FY 2015) for Secure Communities; and National Conference of State Legislatures, "State Actions Regarding E-Verify" (Denver, CO: National Conference of State Legislatures, no date), for E-Verify.

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Annex 2: Instrumented (2nd Step) Regression: Effect of US Deportations of Convicts on Origin Countries' Homicide Rates (WHO data)

	homicide rates (WHO)				
	(1)	(2)	(3)	(4)	(5)
deportation rate (convicts)	0.096	0.16**	0.19**	0.16**	0.21***
	[1.19]	[2.17]	[2.14]	[2.26]	[3.23]
deportation rate (non conv.)			-0.0033		
			[-0.13]		
corruption		0.15	0.23	0.44	0.21
		[0.3]	[0.42]	[0.39]	[0.19]
exposure US crime		0.019	0.029	-0.0068	0.024
		[0.47]	[0.74]	[-0.16]	[0.45]
GDP growth		0.035	0.011	0.05	-0.013
		[0.81]	[0.22]	[0.75]	[-0.09]
GDP per capita		3	6.1	5.6	14
		[0.59]	[1.15]	[0.88]	[1.42]
gini (market)		0.17	0.2	0.23	0.26
		[1.06]	[1.26]	[0.74]	[0.47]
polity		-0.11	-0.13	-0.078	-0.44
		[-0.81]	[-0.84]	[-0.56]	[-1.21]
population (log)		8.4	5.2	4.7	-14
		[1.43]	[0.91]	[0.41]	[-0.62]
remittances		0.54*	0.66**	0.73**	0.98
		[1.85]	[2.09]	[2.01]	[1.57]
schooling		-0.76	-0.75	-1.6	-2.1*
		[-1.35]	[-1.37]	[-1.52]	[-1.83]
share population < 14		0.62	0.65	1	1.7*
		[0.94]	[1.01]	[1.19]	[1.71]
urban		0.58**	0.57**	1.3***	1.1
		[2.35]	[2.38]	[2.71]	[1.35]
US arms imports		1.20E-04	2.60E-04	-0.022*	-0.027
		[0.35]	[0.73]	[-1.65]	[-1.29]
sample	full	full	full	w/o OECD	Lat. Am & Caribbean
country fixed effects	yes	yes	yes	yes	yes
year fixed effects	yes	yes	yes	yes	yes
R^2	0.02	0.2	0.2	0.27	0.33
weak instr. F-test	21.54	26.58	32.81	37.64	31.76
Wu Hausman	0.68	0.29	0.03	0.1	0.07
Sargan	0.93	0.93	0.93	0.93	0.93
#obs	515	347	322	169	112
periods (max)	7	7	7	7	7
countries	90	65	65	37	22

Heteroscedasticity robust t-values in parenthesis. All results are based on 2SLS regressions with country and year fixed effects for 2-year intervals. Stars denote significance at the 10% (*), 5% (**) and 1% (***) level.

Annex 3: Instrumented (2nd Step) Regression: Effect of US Deportations of Convicts on Origin Countries' Homicide Rates (UNOC data, yearly intervals)

	homicide rate				
	(1)	(2)	(3)	(4)	(5)
deportation rate (convicts)	0.23***	0.23*	0.28*	0.23**	0.32***
	[2.59]	[1.8]	[1.82]	[1.97]	[2.91]
deportation rate (non conv.)			-0.072		
			[-1.35]		
civil war (=1)		-1.9	-1.4	-1.5	
		[-1.47]	[-1.16]	[-1.03]	
corruption		-0.24	-0.16	-0.54	-0.14
		[-0.43]	[-0.27]	[-0.47]	[-0.07]
exposure US crime		-0.033	-0.018	-0.07*	-0.14**
		[-1.13]	[-0.59]	[-1.69]	[-2.21]
GDP growth		-0.058	-0.042	-0.11	-0.31
		[-1.43]	[-1.17]	[-1.54]	[-1.65]
GDP per capita		4.2	3.7	6.9	20
		[1.01]	[0.92]	[1.07]	[1.16]
gini (market)		0.26	0.17	0.46	1.2*
		[1.22]	[0.89]	[1.25]	[1.83]
polity		-0.075	-0.1	-0.0075	-0.29
		[-0.49]	[-0.74]	[-0.05]	[-0.83]
population (log)		13	14**	20**	55
		[1.65]	[2.18]	[2.08]	[0.83]
remittances		-0.32	0.016	-0.34	-1.3
		[-0.65]	[0.07]	[-0.64]	[-1.4]
schooling		-0.53	-0.6	-1.2**	-1.3**
		[-1.35]	[-1.61]	[-1.95]	[-2.04]
share population < 14		1.2*	1.1*	1.5	6.5***
		[1.72]	[1.67]	[1.62]	[3.29]
urban		0.16	0.22	0.17	-0.18
		[0.65]	[0.89]	[0.36]	[-0.29]
US arms imports		-3.80E-05	2.70E-04	-2.10E-04	-1.30E-03
		[-0.09]	[0.84]	[-0.02]	[-0.09]
sample	full	full	full	w/o OECD	Lat. Am & Caribbean
country fixed effects	yes	yes	yes	yes	yes
year fixed effects	yes	yes	yes	yes	yes
R^2	0.15	0.25	0.29	0.31	0.47
weak instr. F-test	44.23	40.28	29.51	26.67	26.67
Wu Hausman	0.02	0.05	0.03	0.13	0.13
Sargan	0.01	0.01	0.01	0.01	0.01
#obs	1096	653	617	363	170
periods (max)	12	11	11	11	11
countries	113	79	79	51	21

Heteroscedasticity robust t-values in parenthesis. All results are based on 2SLS regressions with country and year fixed effects for 1-year intervals. Stars denote significance at the 10% (*), 5% (**) and 1% (***) level.

Annex 4: Instrumented (2nd Step) Regression: Effect of US Deportations of Convicts on Origin Countries' Homicide Rates (UNOC data, 3-year intervals)

	homicide rate				
	(1)	(2)	(3)	(4)	(5)
deportation rate (convicts)	0.26***	0.14	0.1	0.15	0.23***
	[3.04]	[1.41]	[0.98]	[1.45]	[4.1]
deportation rate (non conv.)			-0.079*		
			[-1.85]		
civil war (=1)		-4.5	-4.2	-3.7	
		[-1.64]	[-1.52]	[-1.2]	
corruption		0.57	0.92	0.24	0.033
		[1.01]	[1.24]	[0.16]	[0.01]
exposure US crime		-0.056	-0.039	-0.11*	-0.28***
		[-1.38]	[-1.05]	[-1.82]	[-3.41]
GDP growth		-0.063	-0.075	-0.13	-0.63*
		[-1.06]	[-1.03]	[-1.09]	[-1.7]
GDP per capita		-1.1	-2.5	-1.2	2.5
		[-0.31]	[-0.59]	[-0.21]	[0.17]
gini (market)		0.38	0.38	0.55	1.5
		[1.34]	[1.26]	[1.25]	[1.5]
polity		-0.2	-0.16	-0.079	-0.62
		[-0.78]	[-0.6]	[-0.25]	[-1.24]
population (log)		12	8.5	19	160
		[1.03]	[0.68]	[1.08]	[1.59]
remittances		-0.77	-1	-0.79	-5***
		[-0.92]	[-0.98]	[-0.82]	[-2.85]
schooling		-0.5	-0.51	-1.2	-2.1
		[-0.84]	[-0.81]	[-0.89]	[-1.19]
share population under 14		0.035	-0.33	0.47	9.7***
		[0.05]	[-0.38]	[0.41]	[3.27]
urban		0.47	0.44	0.84	-2.2**
		[1.43]	[1.3]	[1.63]	[-2.47]
US arms imports		0.0023	0.0021	0.012	0.041
		[0.81]	[0.7]	[0.71]	[1.23]
sample	full	full	full	w/o OECD	Lat. Am & Caribbean
country fixed effects	yes	yes	yes	yes	yes
year fixed effects	yes	yes	yes	yes	yes
R^2	0.16	0.34	0.38	0.38	0.73
weak instr. F-test	12.94	14.76	12.74	12.04	12.04
Wu Hausman	0.48	0.55	0.28	0.32	0.32
Sargan	0.81	0.81	0.81	0.81	0.81
#obs	351	189	178	107	49
periods (max)	4	3	3	3	3
countries	113	72	72	44	18

Heteroscedasticity robust t-values in parenthesis. All results are based on 2SLS regressions with country and year fixed effects for 3-year intervals. Stars denote significance at the 10% (*), 5% (**) and 1% (***) level.

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